

second law of thermodynamics is not violated, as it would be if the diaphragm effected *only* an unequal distribution of energy. The great point, however, to notice is that the sifting power of the diaphragm enables us to derive work from the gas at the expense of its heat, or we obtain thereby a capacity for work without the performance of work, which is the *practical* result we require, and so long as we obtain this result, we may not care so much about any inquiry whether a certain statement of a law is thereby violated or not (at least this inquiry is of *secondary* importance). The main point evidently is to realise how work may be derived from normal temperature heat without a source or refrigerator. Also it cannot surely be kept too much in view that the "second law of thermodynamics" is not *theoretically* a necessary truth, but its truth only depends (as Prof. Maxwell showed) on our inability to grasp or handle molecules. For if molecules were of such a size that we could handle them separately, then there is no doubt that we could transfer their motions to masses (without the necessity for mixing the molecules of different masses together). The attempts to prove the second law of thermodynamics as an abstract truth independently of considering the *molecular* state of matter, can therefore scarcely be considered as legitimate, as it is upon the *molecular* state of matter that the impracticability of the effect expressed in the law depends. Also it would be, perhaps, difficult to give a perfectly satisfactory *a priori* proof that no process can be discovered for utilising normal temperature heat without permanently mixing or altering the distribution of the matter concerned, more especially when it is considered how much can be already done in the way of manipulating molecules (or sifting their velocities) and deriving their heat by means of porous diaphragms in the case of diffusion. The practicability of the result would admittedly not be contrary to the principle of the conservation of energy.

6. One of the most important considerations, perhaps, connected with diffusion, would appear to be that the tendency to the uniform diffusion of matter, or rather of *velocity* [since chemically different molecules of *equal* mass do not necessarily tend to become uniformly diffused], can upset the tendency to the uniform diffusion of energy, *i.e.*, energy could not be uniformly diffused until matter (capable of diffusion) was also uniformly diffused, or homogeneous. Another important consideration would appear to be (and which, if noticed, would seem to be worthy of greater attention) that the gases of the atmosphere from the fact of their being of *different* molecular weights, tend *forcibly* to become uniformly diffused, or the danger of unequal mixture is averted, which would inevitably occur sometime or somewhere, if the gases were of the *same* molecular weights (or *dynamically* alike), and so diffusion were left to the pure contingencies of chance. I may return (by permission of the Editor) to this point at a future opportunity.

S. TOLVER PRESTON

"Underground Temperature"

THE Report of the British Association Committee on Underground Temperature appearing in NATURE, vol. xvii. p. 476, gives me an opportunity of questioning the treatment of the matter and urging the rejection of any figures obtained. This opportunity I had often wished for when reading over the allusions to underground temperature which spoil text-books, but would scarcely avail myself of now were not the "report" apparently "accepted" in significant silence.

To any one familiar with the state of circumstances down in mines, who has accumulated thought on the question of the temperature of the rocks in depth, the observations noted in this report must appear, not to say absolutely inadequate to further the inquiry, but altogether missing the point of it—that is to say, if I am right as I take it, that the purpose of the Committee is to ascertain, not mere "underground temperature" readings, but the proper temperature of the rocks as due to intra-terrestrial conditions. In all the observations conducted in mines, the temperature of the mine ventilation or a temperature almost wholly inter-dependent is expressed by the figures obtained; these figures no more indicate the true state of the case, and are therefore of no more value to the geologist or the general physicist, than the temperature of a greenhouse would assist the meteorologist. The figures obtained in bores express the temperature of the waters standing therein—which temperature cannot at all be assumed to be coincident with the rock temperature; these figures are determined by a variety of factors, the true rock temperature not

being necessarily the greater. In fact, we can never arrive at the temperature of the rocks in depth through the media of water or air. To state this truth is to establish it.

A few remarks, however, bearing directly on the details of the observations in the report, may aid inquirers to arrive at a true state of thought on the subject. In the first place, and as of general application, I have to demand attention to the matter of the mine area, to depths exceeding the observation stations, being depleted of the waters naturally appertaining thereto—and for this reason even a rock temperature ascertained over such an area would be abnormal. A thermometer hung up in a mine way will unquestionably register the temperature of the ventilation in that particular place; nobody expects any other result. Can the temperature of the ventilation be demonstrated to be inter-dependent in a great measure on the temperature of the surrounding rocks, which is itself abnormal as above submitted? Certainly not! and this fact is so obvious to those having a true acquaintance with mine ventilation, that it seems to me too absurd to elaborate proofs of it. Particular stress is laid on the point that currents of air and well-aired situations were avoided. It is hard to see any outcome of this other than still more abnormal readings. Surely observers do not imagine they can penetrate the rocks so far by cornering in mines as to leave the atmosphere wholly behind. Figures obtained in still air express the temperature of dying ventilation, or of gases of exudation, or partly of both. As to the employment of a few inches of water in a hole, as supposed to secure more direct contact with the rocks, or to isolate from air; the temperature of the small body of water so employed is that obtained, and it is obvious it depends on the air temperature, and (worse) the water may possibly be decomposing. These remarks apply, for the most part, alike to the ingenious method employed down Boldon Colliery as to the more simple method of Schemnitz. Then, with regard to the Boldon Colliery observations in particular, the curious in these matters will be led to speculate as to what was going on up the ten-foot hole, and whether the "stagnant" ventilation of the district was not tending towards the explosive conditions. All the circumstances previously set down, supplemented by the exudation of gases, and the subsidence of strata following on coal working, combine to render this one of the unhappiest conceivable situations for the research on rock temperature.

A thermometer down a mine is of no utility beyond qualifying barometric readings.

The bore observations are, I venture to think, a crude phase of the method which may lead to success ultimately. As hitherto conducted, they are open to many obvious objections, which, if stated at length, would be little more than a reiteration of the above in part.

I will venture to suggest that the next steps in advance be the permanent placing of instruments in deep bores, broken rock being rammed over as over powder in blasting operations, so that all water and air, except such as may be fairly considered as entering into the structure of the rocks, be entirely excluded, and the application of thermo-electric apparatus devised by specialists in electric science, all constituting a special, and, for many reasons, invaluable attachment to an observatory or kindred institution.

WILLIAM MORRIS

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Helmholtz's Vowel Theory and the Phonograph

THE results obtained by Messrs. Jenkin and Ewing in their experiments with the phonograph, as described in NATURE, vol. xvii. p. 384, are so different from those reached in some experiments recently performed by Dr. Clarence J. Blake, of this city, in connection with myself, that I venture to call attention to the fact.

With the design of testing the question of change of quality in vowel tones by increasing the rate of rotation of the phonograph cylinder, we performed a number of experiments, of which I mention a few as briefly as possible.

1. The vowels *ou* and *ō* were spoken into the mouth-piece of the instrument, each four times in succession, while the cylinder was rotated at the rate of one revolution per second, as timed by the beats of a clock-pendulum. On rotating the disc so as to reproduce the vowel-sounds, these were as spoken, *ou*, *o*, each repeated four times, when the rate of rotation was one revolution per second, but on increasing the velocity to two revolutions per second, the first sounds were indistinct, while the last gave the

vowel ϵ very clearly. At half revolution per second, *ou*, *au*, were distinctly heard.

2. The vowel \bar{o} was sung while the cylinder rotated at different rates of speed. On reproducing the sounds, the cylinder being revolved more slowly than at first, the vowel *au* was heard, changing to \bar{o} , ϵ , \bar{e} , falling to ϵ again as the velocity was slackened a little.

3. The vowel \bar{a} was spoken while the cylinder made one revolution per second. On reproducing the sound, the rate being half a revolution per second, *au* was heard, changing to \bar{a} when the rate increased to one revolution, and at three revolutions per second \bar{a} was heard.

4. The vowel \bar{o} was spoken several times in succession, the rate of the cylinder being gradually accelerated. On reproducing the sound by a uniform and slow rotation, *au* and *ou* were heard; on rotating faster, \bar{e} and \bar{z} .

Several other experiments were tried in the short time during which the instrument was at our service, all of which were strikingly confirmatory of Helmholtz's theory. Difficulty was experienced in reproducing the highest vowels \bar{e} , \bar{z} , probably on account of want of readiness of response in the disc. The bell of a reed-pipe was placed over the mouth-piece of the instrument when the sound was to be reproduced, for which a horn of pasteboard was substituted in some of the trials.

We hope to render these experiments more rigorously quantitative, as the phonograph promises to be a valuable aid to research in this field. Very probably others may have worked with the same end in view, and if so it would be interesting to learn what has been their experience.

CHAS. R. CROSS

Boston, U.S., April 29

The Telephone

WITH reference to the letter of Lieut. Savage which appeared in your last impression (p. 77) respecting the telephone, this gentleman has noticed that on removing the ferrotype disc of the sending instrument and tapping the magnet with a diamagnetic body, such as a piece of copper, the taps are distinctly heard at the receiving end. I have repeated this experiment. Not only can a diamagnetic substance be used for tapping, but the magnet may be removed altogether and a bar of soft iron substituted without causing any material difference in the results, and this bar of soft iron may be placed at right-angles to the line of dip. The vibrations of a tuning-fork are transmitted very distinctly. When held in the line of dip the results obtained are more marked. Taps and the tuning-fork vibrations are readily heard, and by covering with the ferrotype disc a conversation was actually carried on through this bar of soft iron. There is perhaps nothing very surprising in obtaining these phenomena with the bar in the "dip" line, but when the same bar of perfectly soft and recently annealed iron can be held in any position in a plane at right-angles to that line and used as a sender for powerful vibrations, such as those of a tuning-fork or the taps of a diamagnetic body on the naked end of the bar, we cannot but be struck by the surprising delicacy of the telephone as a test for the earth's magnetism.

The receiving instrument used in the above experiments was an ordinary bell telephone $2\frac{1}{2}$ in. disc '007 of an inch in thickness.

9, St. John's Road, Bristol, May 18 ALFRED CHIDDEY

Hereditary Transmission

IN 1837, Capt. D'Urban of H.M.S. *Griffin*, having captured, off the south coast of Martinique, a Portuguese slaver, called the *Don Francisco*, landed in this colony the living freight of 437 human beings, who, about two months previously, had been forced from their homes on the banks of the Congo, to be sold in Cuba.

William Laidlaw, one of the liberated slaves, who is now in a position of some trust on the Goodwill sugar plantation in the island, gives to me the following interesting details of hereditary transmission in the African, which I believe will be interesting to the readers of NATURE.

"I am about sixty or sixty-five years of age, and was born with six fingers on each hand. Soon after 'my freedom' I married a woman from 'our country.' We had four children, two being boys and two girls; they were born with six fingers on each hand, and one of the girls had six toes on each foot.

"My eldest son Robert, who is married and settled in Demerara, is the father of two boys, who have six fingers on

each hand. My second son, William, who is working with me on the Goodwill estate, married, and his wife had five children; they were born having the same peculiarity; but I regret to say none are living."

I yesterday sent for William Laidlaw, and have substantiated his father's statements. I measured the sixth fingers: the one on the right hand is exactly $1\frac{1}{4}$ inch in length, and has a perfectly formed nail, the one on the left showed traces of having been partially amputated.

EDMUND WATT

Resident District Magistrate

Dominica, British West Indies, April 27

What is a "Water-shed"?

SOME time ago the term "water-shed" was somewhat vaguely used to imply either the dividing ridge between two river basins or the slopes down which the water poured into the rivers themselves. Latterly, if I mistake not, it has generally been used by geographers in the former sense only. Mr. George Grove, F.R.G.S., however, in his excellent little Primer on Geography, uses the term "water-parting" for the ridge, and water-shed for the whole of the ground between the water-parting and the stream;—very clearly illustrating his meaning by reference to the ridge tiles and the slope of the roof of a house respectively.

There may be some reason, especially in a work of the kind, for substituting "water-parting" for "water-shed," in the sense first quoted, but is the use of the latter, to indicate the flow of water down the slopes, justified either by etymology, or even by the correct use of the word "shed" in ordinary conversation?

The derivation from Anglo-Saxon *scad-an* or *scad-an*, indicates the primary meaning to divide or sever. It is also used metaphorically in some of the north-country dialects, as "there is no shed (difference) between us." No doubt, by a very natural ellipsis it often implies flowing or falling. A woman sheds tears, or a tree sheds its leaves, and the consequent flowing down the cheeks, or fluttering down to the ground need not be specially expressed. But in this case the word is used distinctively, and should surely be used, if used at all, in its stricter and primary sense, while the fall or flow of water can be appropriately distinguished.

Of course this is merely a question of terminology, but I think it is one worth noticing if only for the sake of the youthful millions who are being brought to some knowledge of elementary geography, and will hardly be helped to appreciate the exactness of science if they find the same word is used by different authorities to describe things so different as the dividing ridge and the hill slopes of the land they live in.

R. H.

Savile Club, Savile Row, W.

Abnormal Coccyx

IN NATURE for September 21, 1876, I gave an account of a peculiar abnormality in a girl aged eight, in whom the coccyx was turned backwards and upwards, and a little above it there was a circular depression in the skin, about $\frac{1}{4}$ inch in diameter, and about $\frac{1}{4}$ inch deep. On being dragged downwards the skin in this hollow became everted and formed a covering to the point of the coccyx. Shortly afterwards I had an opportunity of examining the other children of the family, with the following results:—

Boy aged six, normal.

Girl aged four, depression in the same spot as in the eldest sister, coccyx normal.

Girl aged two, normal.

Boy aged seven months, fairly deep hole (not measured) in same position, coccyx less curved forward than usual.

The parents were said not to possess this peculiarity; I could get no information as to the other members of the family.

A few days ago I met with another case of the same kind in a boy eight months old. The coccyx was curved sharply backwards, and there was a circular depression in the skin, about 5 mm. in diameter, a little higher up than in the other cases, which was easily raised to the level of the surrounding parts, and effaced by a little traction.

ANDREW DUNLOP

Jersey

Lecture Experiment

A glass flask of about a litre capacity is partially filled with water and closed with a cork, through which a tube passes